American Meteorological Journal.

A Monthly Review of Meteorology
—AND—
Allied Branches of Study.

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THE AMERICAN

Meteorological Journal.

VOL. I. DETROIT, SEPTEMBER, 1884. NO. 5.

EDITORIAL NOTE.

IT is not worth while to consider whether the deficiency of observations on local storms, which makes the determination of their action doubtful, could not be remedied by appointing special days on which hourly or bi-hourly observations should be taken, with additional records at still more frequent intervals when any change in the condition of the air required it. These special days might be on certain pre-arranged dates, "term days," so called, when the records would gather up anything that happened to come along in the passage of the weather; but they would better serve the purpose here in view if they were really specially appointed by the Signal Service officers, only a day or two before their date. It is evident enough from an inspection of Finley's maps and from a brief study of summer thunder-storms that the southern side or southeastern quadrant of our passing cyclones contains the greatest share of local disturbances. Let the plan be published in advance by circulars and newspaper paragraphs, and then if, while a cyclone was still beyond the Rocky Mountains, the day of its arrival over the Upper Lakes could be foretold, there might be thirty to sixty hours telegraphic notice given of the appointment of such a day for special observation over the whole region east of the Mississippi. The notice should properly take a somewhat striking form so as to excite an in-

terest in the attempt among people who would ordinarily let the weather changes pass by almost unnoticed; the newspapers and railroads could be in nearly all cases counted upon to aid in spreading the news of the appointment; and even if the general records gave only the direction and estimated force of the winds and the beginning and ending of rainfall, two or three special days of observation in June or July, might produce a wonderful fund of material for study.

CURRENT NOTES.

M. FAYE has claimed that the scourge of lightning has been subdued by the lightning rod. The *American Exchange and Review* for July gives a tabulated account of fires in Massachusetts for 1883. From this we gather that lightning was known to cause 31 of the 2,233 fires tabulated. Of these 16 were barns and 11 dwelling houses. The scourge is well known to be worse in country districts than in town, where the systems of pipes and wires give abundance of good ground connections.

DURING the rain and thunder storm on the morning of July 24, the barometric changes, as shown by the self recording instruments of the observatory, were very marked. The figures here given were obtained by scaling the sheet on which the record was made.

ANN ARBOR M. T.	PRESSURE. Inch.	ANN ARBOR M. T.	PRESSURE. Inch.
$6^h\ 0.^m$ A. M	28.94	$8^h 30.^m$ A. M	29.01
15.	.94	45.	.03
30.	.94	9 0.	.03
45.	.96	15.	.07
7 0.	.97	30.	.07
15.	.98	45.	.05
30.	28.99	10 0.	29.01
45.	29.00	15.	28.96
8 0.	.01	30.	.93
15.	.01	45.	.94

During the ten hours preceeding 6 A. M., and the twelve hours following 11 A. M., the pressure remained constantly at $28\overset{\text{in.}}{.}94 \pm 0\overset{\text{in.}}{.}01$.

At $9^h\ 10^m$ A. M., the pressure was increasing at the rate of $0\overset{\text{in.}}{.}01$ per minute. At about $10^h\ 2^m$ the rate of decrease was $0\overset{\text{in.}}{.}04$ in one minute of time. From 8 A. M. to 10 A. M. the rainfall was $0\overset{\text{in.}}{.}45$.

THE unusually bright sunsets seem to be recurring in this latitude. They have been observed occasionally during June, July, and August.

THE *Boston Transcript* for July 19, 1884, notes that on the preceding afternoon a shower at Chestnut Hill reservoir gave the heaviest rate of rainfall for more than five years. According to Mr. Desmond Fitzgerald's self-recording gauge, at that point 0.72 of an inch fell in seven minutes. The whole shower yielded 1.19 inches. The fall was at the rate of over 6 inches per hour.

THE *Monthly Weather Review* gives authentic information concerning the May floods in Texas. Newspaper readers will remember the extraordinary stories, in the papers at that time, of repeated cloudbursts, floods so extensive as to wash away railroad bridges and stop traffic, of rivers which temporarily changed the direction of their currents on account of the great and sudden inflow of water, and of immense destruction to crops and stock. The official figures of the Signal Service indicate the probable truth of these reports. The rainfall on the average was from two to three times the mean for the Western Gulf, Rio Grande Valley and Southern Slope. For a small region about Palestine, Texas, it surpassed 17 inches and at Weatherford, Texas, the figures were reported at 27.94 inches, though the report expresses the doubts of the compilers by the use of an interrogation mark.

A curious feature of this excessive precipitation is that it seems to have no relation to the mean isobars and isotherms. The pressure for Texas lay between the highest and lowest, the former lying over Georgia, the latter over Arizona. The isotherms for the month show no noteworthy irregularities. The general storm areas also seem to have avoided Texas for this month. But a single one of those traced touched the State, and that seems to have originated in its extreme northern part. The fact seems to be that this extreme precipitation was a part of what are called local storms. It is in accordance with experience, though attention has not been sufficiently called to it, that the heaviest rainfalls occur with local disturbances. In this month hailstorms, cloudbursts, and tornadoes were reported almost daily from Texas. Later news informs us that the heavy rainfall continued in Texas into June and then ceased, and that now (early in August) a drouth is prevailing there.

Fig. 2, page 124, of our August issue, would perhaps be more intelligible if it were so turned that the scale were at the bottom of the cut.

Commander Bartlett of the Hydrographic Office contributes the following extract from a report received from the New York Branch August 13th:

"Bark Stillwater of St. John, N. B., Yondez, Manilla, 131 days with sugar, passed through large quantities of floating pumice and debris of the volcanic eruption from Krakatoa for 3500 miles in a southwestern direction, or until off the south end of the Madagascar."

In an early number of the Meteorological Journal, attention was called to the importance of careful definition of certain terms commonly in use by observers. The following paragraph, now going the rounds of the newspapers, has a similar bearing.

Cyclones.—Professor W. H. Brewer, of the Sheffield Scientific School, says: "Most people have the idea that the word 'cyclone' expresses something much more terrible in the way of a wind storm than either the words 'whirlwind' or 'tornado,' whereas the reverse is the case. A cyclone extends over a wide circuit, and need not be violent enough to cause any serious damage through the larger part of its path. We generally have two or three cyclones a month, but hardly ever a tornado. The wind storms that are of such common occurrence in the West, and sometimes visit this section, doing so much damage to property and life, are not cyclones at all, although usually part of one. The tornado has a narrow path and the cyclone a wide one. The old English word is whirlwind. Some forty years ago meteorologists in the United States adopted the Spanish word 'tornado' as a synonyme for it. Tornado soon found its way into American dictionaries, but has never yet appeared in the English dictionaries. Lately the word 'cyclone' has come to be applied to these violent and destructive storms, and, owing to its acceptance by the newspapers, has been popularized. But it is, nevertheless, an improper use of the word, and should, therefore, be discouraged by the press."

Any good effect that this suggestion may have, will probably be neutralized by the following, which appeals much more strongly to the scissors of the 'local editor.'

Five Cent Cyclones.—Prof. Douglas, of the Michigan State University, it is said, produces amateur cyclones at will. He does it by suspending a large copper plate by silken cords. This plate is charged heavily with electricity, which hangs down like a bag underneath, and is rendered visible by the use of arsenious acid gas, which gives it a green color. The formation is a miniature cyclone, as perfect as any started in the clouds. It is funnel-shaped and whirls around rapidly. Passing this plate over a table, the five cent cyclone snatches up copper cents, pens, pith balls and other objects and scatters them on all sides. W. M. D.

[As there is no Prof. Douglas in the University of Michigan, we fear the above is apocryphal.—Ed.]

THE following note we owe to the Hydrographic Office. It is an extract from the report of Lieut. W. P. Ray, U. S. N., in charge of the Branch Hydrographic Office, New Orleans, La.:

"Captain C. W. Reed of the "City of Dallas" reports that all the captains cruising along the east coast of Yucatan and northeast part of Yucatan have been very much surprised at the absence of the usual northerly current during April, May, June, and July. There has been no perceptible current until the last three days. The sailing directions give one and a half to two and a half knots per hour for these months."

IN the matter of definitions, there is need of further care is distinguishing *tornadoes* from *squalls*. Both are sudden in coming, violent and short-lived at any one point, and they are alike in being accompanied by heavy rain and vivid lightning, but further than this they seem to differ. The typical examples of *squalls* (German, *Böen*) that have been decribed in detail by Hinrichs, in the Iowa Weather Bulletin for June, 1881, and by Köppen in the Annalen der Hydrographie, XII, 1882, 595, 714, show them to be straight line gusts, blowing persistently almost in a constant direction, occurring over a long strip of country that stretches at right angles to the course of the wind, and advancing broadside at a rapid rate about in the direction of the wind blowing. Whirling winds, essential in the tornado, are here only incidental and not at all characteristic. The description given by Köppen of the squall in northern Germany on August 9, 1881, shows that it was dependent on cyclonic winds for its opportunity, as tornadoes are, but that it occurred closely along the line of strongest temperature gradients, instead of further east within the area of the warm surface winds, where Finley finds the tornadoes; so that there seems to be ground for the separation of these two kinds of wind in their attitude as well as in their action. Corresponding to this difference of attitude it is natural to expect a difference of explanation; tornadoes are now generally considered the result of a too rapid *vertical* decrease of temperature; Köppen suggests that squalls result from a too rapid *lateral* change of temperature, coming from the close approach of cool westerly to warm southerly winds, with an approximately vertical plane of separation; the squall is then the sliding of the cool air under the warm air, giving somewhat the appearance of rotation on a horizontal axis, while the tornado has a well pronounced rotation about a vertical axis; and these rotations are in opposite direc-

tions. It is very probable that the special records by the volunteer Signal Service observers of this year under the direction of Messrs: Finley and H. A. Hazen will give additional examples of local storms that will come better under the name of squalls than under either tornadoes or thunder-storms. W. M. D.

A REPORTER of the Cincinnati *Enquirer* picked up from a German American, not long ago, a series of weather prognostics by animals, some of which are not given in Signal Service Notes No. IX. The following are the more interesting and we give them for what they are worth.

In a large glass jar that sat in a window-sill of an Eighth street residence an *Enquirer* man saw a small ladder, not over a foot long, reaching to the top, and two tree-toads frisking about in the water which about half filled the jar. An old German, spectacled and dressed in a long double-gown, was eagerly watching the motions of the batrachians. "Those are queer pets, doctor," remarked the scribe. "Pets! They are not pets. They are my barometers. They tell me with accuracy when the weather is to be fair or foul. If dry weather is coming on, they climb the ladder away out of the water, but if it is going to rain, then down they go into the water." * * * * * There's the snail. I do not know of any surer way of predicting the changes in the weather than by observing their habits closely. They do not drink, but imbibe moisture during a rain and exude it afterward. This animal is never seen abroad, except before rain, when you will see it climbing the bark of trees and getting on the leaves. The tree-snail, as it is called, two days before a rain will climb up the stems of plants, and if the rain is going to be a hard and long one, then they get on the sheltered side of a leaf, but if a short rain, on the outside. Then there are other species that before a rain are yellow; after it blue. Others indicate rain by holes and protuberances, which, before a rain, rise as large tubercles. These will begin to show themselves ten days before a rain. At the end of each tubercle is a pore, which opens when the rain comes, to absorb and draw in the moisture. In other snails deep indentations, beginning at the head between the horns, and ending with the jointure at the tail, appear a few days before a storm.

"Take the ants. Have you never noticed the activity they display before a storm, hurry-scurry, rushing hither and yon, as if they were letter-carriers making six trips a day, or expressmen behind time? Dogs grow sleepy and dull, and like to lie before a fire when rain approaches; fowls roll in the dust.

"Beetles flying late in the evening foretell a fine day on the morrow; cranes flying high. Hogs when they run squawking about, and won't bite, and jerk up their heads, and see a wind coming, hence the proverb, 'Pigs can see the wind.'"

When you see a swan flying against the wind, toads coming out of their holes in unusual numbers of an evening, worms, slugs, and snails appearing, robin redbreasts pecking at our windows', you can put them all down for rain signs.

Some of these signs seem to be imported from the old country and our confidence in the German is somewhat shaken by his attributing Dr. Jenner's "Signs of Rain" to Dr. Darwin.

THE following is taken from the *Monthly Weather Review* for May. It is of interest in connection with the discussion now going on, concerning the relations between local storms and pressure.

El Paso: at 12.36 p. m., of the 27th a violent whirl-wind passed over this town, carrying with it a column of sand more than one hundred feet high. The diameter of the whirl was apparently from ten to fifteen feet, and its rotary motion from right to left. Its path was almost in a direct line from west-southwest to east-northeast, and in the vortex many light objects were carried to a height of about eighty feet. When the whirlwind reached a large shed in its course it became stationary for a short interval; the rotary motion increased in violence, tearing off a part of the sheet-iron roof and hurling it some distance. Several plates of sheet-iron of from seventy-five to one hundred square feet of surface were taken upward to a height of forty feet and carried a distance of one hundred feet. No unusual change was noted in the meteorological instruments during the passage of the whirlwind.

THE best method of protecting thermometers is now attracting world wide attention. We are surprised at a recent report of a Thermometer Service Committee, to the Royal Meteorological Society. The Committee seem to have decided that the Thomson shelter was the best to be used, notwithstanding all that has been said against the shelter by Prof. Mohn, and Prof. Wild. This shelter is only 18 inches long, 11 wide, and 15 high. Its principal characteristics seem to be sufficient closeness to keep out all rain and a feeble attempt at vertical circulation. Neither of these points are essential. At the time of rain wet and dry thermometers read alike, and no harm can come from wetting the dry. Certainly introducing a constant error of 2 or 3 degrees in clear calm weather and making all hygrometric observations absolutely valueless, from a lack of circulation, can hardly be commended, if the object is merely to provide against a wetting a dozen times a year. It can be shown that a vertical circulation is of no importance whatever; a perfect horizontal circulation is all that is needed. No shelter can be regarded satisfactory that has not the following points:—

1. A perfect circulation of air currents; nearly everything should be sacrificed for that, after shielding from the sun's direct rays. This is essential, even if artificial ventilation be employed.

2. There should be a clear space of 3 inches between the inner and outer roofs with nothing to impede air circulation in all this space. Boring holes in the inner roof is useless and objectionable. Air cannot become stagnant at the top of the screen, as it must work out between the louvres, and as it would be impossible to keep rain from trickling down through the holes, even in

calm weather, an ordinary single louvre would be less objectionable since it would keep out all rain save in brisk winds.

3. The screen should allow a space of at least 18 inches between the thermometer and louvre. Experiment has shown that in clear calm weather a thermometer 6 inches from a louvre will indicate at times a temperature, 1° higher than if 18 inches. This however will only be the case with a good circulation, in a close screen but little difference would be experienced. H. A. H.

IN THE Quarterly Journal of the Royal Meteorological Society, for April we note an interesting paper on the great storm that crossed the British Islands on January 26, 1884. The lowest pressure was $27^{\text{in}}.33$ experienced at Ochtertyre at 9.45 P. M., and is the lowest recorded pressure ever noted in Britain. "Thunderstorms occurred on the southeast side of the depression and traveled east at 30 miles per hour across the south of Ireland and the greater part of England to the east coast. These thunderstorms were most probably associated with a subsidiary depression." The gale extended to the south of France and to Switzerland. The velocity of the wind by registering instruments was at Valencia 68 miles, 26th, 11 A. M., Holyhead 70, 2 P. M., Armagh 69, 5 P. M., Aluwick 76, midnight. We note a surprising error in a supposed range of air pressure of 5.771 inches at Ben Nevis. It is safe to say that no such range has ever been noted at sea level, (much less at a high station), and never will be. The error has probably come from using a sea level value (28.944) of an observation at Ben Nevis and comparing it with an actual value 23.173. Allowing for reduction the first would be 24.47, showing a range of $1^{\text{in}}.30$. A pressure of $28^{\text{in}}.944$ at Ben Nevis, 4,406 feet above sea, would give a sea level value of about $33^{\text{in}}.41$. H. A. H.

A LITTLE more than a year ago foreign journals were filled with descriptions of a novel experiment tried by Prof. Lemström, in Sodan Kyla. This experiment consisted in arranging a properly insulated net work of wires, upon a hill some 600 feet in hight and connecting it with a galvanic battery. The result was the appearance of a veritable aurora, its character being checked by noting the auroral (citron) line in the spectrum. Nothing has been heard from Prof. L., thus far this year though he was to try special experiments again last winter. Prof. Sophus Tromholt communicates a note to *Nature* in which we learn that he utterly failed in

obtaining a simulation of the aurora with the same apparatus in Iceland the past winter. He lays the failure to abundance of fog and moisture which at all times was present at his station. This explanation is far from satisfactory however. In the arctic night of the northern regions with no sun to affect the moisture conditions can they be sufficiently different at the two stations to give success at one, and absolute failure at the other?

May it not be that Prof. Lemström's aurora was produced by an actual earth current or an aurora settling upon his wires. Certainly these would have a tendency to attract atmospheric electricity. We shall be pleased to find a confirmation of his first year's work, in the work of the past winter. The subject is worthy of the closest study. H. A. H.

THE FROSTS OF LATE MAY.

The frosts of May always attract great attention on account of their destructive effects on the young crops. Those of late May this year were particularly destructive, and now that the May number of the *Weather Review* of the Signal Service is published, the relations of this extensive frost to the general weather can be definitely ascertained. The frost began in the neighborhood of the upper lakes. On the nights of the 28th, and 29th they were felt in the States of Wisconsin, Michigan, Illinois and Indiana. By the morning of the 29th they had spread eastward to New York and New Jersey, southward to Maryland and Kentucky, and in the west they occupied Iowa. The progression was rapid eastward but slower southward, and most slow toward the west. On the next morning they were felt in western New England from Connecticut to New Hampshire, and occupied central and western Canada. The next day (30th) they had reached Maine, Rhode Island and Delaware, but had relinquished the south and west, except Iowa. On the 31st, they were reported only in New England and New York.

In their progress they had done an immense amount of damage. Tobacco was severely injured, (to a great extent in the north it was ruined) the fruits suffered severe damage, and corn, potatoes, and early vegetables were injuriously nipped. The damage in Connecticut was estimated at $1,000,000. In Bristol county alone, in Massachusetts, the damage was $100,000. In Duchess and Ulster counties, New York, the damage to the grape crop was $100,000, and

that to the strawberry crop on Long Island, $50,000. In Tioga, Co. Pennsylvania, the total damage was upwards of $10,000. Estimates for other places are not at hand, but the total damage could hardly have been less than two and a half millions of dollars. A wave of cold worth so much money as this deserves public attention. The relations between late spring frosts and the general weather are now well known, and this particular case is a good illustration of this relation.

It is very plain that the frosts originated, or appeared, on the upper lakes and travelled eastward. Their westward shift carried them only into Iowa, while eastward they gradually extended to Maine and Maryland. Their rate of progress was rapid toward the east and it took them only about two days to reach the Atlantic. As they passed eastward they gradually relinquished their western territory (except Iowa.)

Their direction of motion and velocity were like those of storm-areas, but on comparing dates and places, we find they correspond not with a storm-area, or a low pressure, but with an area of high pressure. The high pressure which the frosts accompanied was the seventh which had been noted in the United States in this month, and is thus described by the Signal Service:

"VII.—The midnight report of the 26th, showed a slight rise over Lake Superior; during the 27th, the pressure increased rapidly over the lake region, the barometer remaining highest over Lake Superior until the 29th. The area of increasing pressure moved eastward to the Atlantic coast on the 28th. The temperature fell decidedly over the lake region on the 27th, and a slight fall extended southward over Tennessee and the south Atlantic States. The temperature continued to fall in the lower lake region on the 28th, the cold wave extending over New England and the middle Atlantic States, and on the 29th the temperature fell from 4° to 12° in the south Atlantic States."

A comparison of the progress of this area and of the frosts, shows that the latter closely followed the former, and this is generally true of untimely frosts. They accompany a high barometer. The reasons of this are to be found in the character of areas of high pressure. A high pressure occupies an area where the cold upper air is gradually settling. The air flows slowly out at the circumference of the area so that there slight currents of wind are experienced. Elsewhere in the area the air is nearly calm. The descending air brings down with it something of the temperature of outside space, so that the

high area is relatively cool. Besides, the air coming from a cold region where there is little moisture is relatively dry and does not form clouds. Hence the sky is here clear. With the absence of clouds comes free radiation to outer space, and consequent farther fall of temperature. While the sun is up, his radiations make up for the loss by radiation to the sky, but as soon as he sets the loss by radiation is not made up and the temperature falls. This radiation is the greater, because the air is dry and there is no moisture to retain the dark heat radiated from the ground and the leaves of vegetation.

The temperature continues to fall without interruption until it gets cool enough to deposit dew or hoarfrost. When this occurs the condensing moisture releases the heat which has been employed in keeping it in the gaseous state, and this heat retards or stops the fall of the temperature. If now the air is not so dry but that the dewpoint is above 32° there will probably be no frost, but if the dewpoint is below 32° the temperature may fall below the freezing point and a frost be the result. Thus the frosts go with areas of high pressure because these areas are usually cool, clear and dry.

The spread of the frosts follows the spread of the area. When one of these areas is sufficiently dry to carry frosts with it, if we can predict the progress of the area, we can predict the progress of the frosts. Unfortunately the areas of high pressure are much more uncertain and erratic in their motions than are the low, or storm-areas. They frequently stand still, sometimes for days together, and may then remain constant or may grow or decrease. They show a general inclination to travel eastward but they can not be relied on for this, for they may conclude to shift southward or to any other point of the compass. Hence predictions founded on them are not very reliable and we can not predict frosts with the same certainty that we can predict clouds or rain.

There is an unusual fund of interest in an area of high pressure. It brings bright, sunny, clear days, cool in summer, but seeming warm (from the sun's rays) in winter. They are the days which seem most pleasant, and during them the air seems to have a tonic property not experienced at other times. With them also come the days of Indian summer. But while the high area brings bright and attractive weather, it also brings frost and absence of rain. If it tarries drouth and forest fires make their appearance, and when it remains a longer time famine becomes its companion. While it brings calmness, and clear skies of a brilliant blue or delicatley tinted with

haze, and a stimulating air like that of mountains, it also brings some of the greatest of meteorologic scourges.

FINLEY'S TORNADO PREDICTIONS.

The account of tornado predictions given by Sergeant Finley, in the July number of the JOURNAL, is of great interest to those who are sanguine of the ultimate successful forecasting of these destructive storms. In my judgment it shows encouraging progress, and if I take occasion to point out a fallacy in his discussion, the reader must not understand that I undervalue the general results of his investigation.

The percentages of verification of tornado predictions, as given in the final column of the second table, page 87, range for different series from 95.61 to 98.65. The percentages of verification for the daily prediction of the weather by the Army Signal Office, as published from time to time, range from about 85 to about 90. It would thus appear that tornadoes are peculiarly susceptible of precise forecasting, whereas they have heretofore been omitted from the regular reports on account of their supposed difficulty. Evidently an error inheres in the method of estimating the percentage of verification. I am not prepared to say that Mr. Finley's method of estimation differs essentially from that used by the Chief Signal Officer in preparing his reports of verification, for I am not familiar with the latter; but I hope to show that Mr. Finley's method involves a serious fallacy.

This fallacy consists in the assumption that verification of the predictions of a rare event may be classed with verifications of the predictions of frequent events, without any system of weighting. The occurrence of tornadoes in any given one of the districts indicated by him, is highly exceptional; their non-occurrence is the rule; and this consideration is overlooked when the predictions of occurrence and non-occurrence are classed together as of equal difficulty. Taking advantage of this consideration, it is possible to (apparently) equal or even excel Mr. Finley's results, without any study of the meteorological record. In order to obtain a high ratio of verification, one needs only to predict uniformly for each district and each period of time, the non-occurrence of tornadoes; such an

assumed indiscriminating prediction gives for each of the periods published in his article, a higher average verification than claimed by him. I will illustrate by summing his four series.

In the fourth line "549" appears to be a misprint for 540. Making this change and deducing the totals, I find that he reports for the months of March, April and May, 2,803 predictions, of which 100 announced the occurrence of tornadoes, and 2,703 their non-occurrence. 28 of the former were verified, 2,680 of the latter. There were therefore 72 positive predictions unverified; and there were 23 tornado occurrences unpredicted. There were in all 51 tornado occurrences. The total number of verifications of positive and negative predictions was 2,708, or 96.61 per cent. of the whole number of predictions. According to Finley's method of computation, this percentage is the measure of his success. If, now, ignoring the weather maps and considering only the general *a priori* probability, some one had prophesied for each of the indicated areas and time intervals the non-occurrence of tornadoes, his prediction would have been verified in all but 51 cases, giving a total of 2,752 verifications, or a percentage of 98.18!

It is easier to point out an error than to enunciate the truth; and in matters involving the theory of probabilities the wisest are apt to go astray. The following substitute for Mr. Finley's analysis is therefore offered with great diffidence, and subject to correction by competent mathematicians.

Represent the number of positive predictions by p, the total number of tornado occurrences by o, and the number of co-incidences, or the number of verified predictions, by c. If these three quantities are numerically identical, it is evident that the ratio of verification will be unity. If $c=0$, the ratio of verification is also 0. Between these limits fall all practical cases. If the endeavor of the predictor is merely to include in his predictions all tornado occurrences, then the ratio of verification is expressed by the fraction $\frac{c}{o}$. If his endeavor is merely to exclude all non-occurrences, the ratic of his success is expressed by $\frac{c}{p}$. If the endeavor comprises both inclusion and exclusion, the measure of success is some function of $\frac{c}{o}$ and $\frac{c}{p}$ or, more simply, of p, o and c. Let us endeavor to determine this function.

The number of tornado occurrences not predicted is expressed by $o-c$, and this quantity is in some sense the measure of the failure

in inclusion. The number of positive predictions unverified is expressed by $p-c$, and this quantity is in some sense the measure of the failure in exclusion. If inclusion and exclusion are equally important, their measures bear the same weights, and $o+p-2c$, the sum of their measures, is a measure of the general failure of prediction. In a similar sense c is a measure of the verification of prediction, and the ratio of verification, v, is expressed by (the favorable cases divided by the sum of the favorable and unfavorable)

$$v=\frac{c}{(o+p-2c)+c}=\frac{c}{o+p-c} \quad \cdot\cdot\cdot\cdot \quad (1).$$

In the case of Mr. Finley's work for March, April and May, $o=51$, $p=100$, and $c=28$. If he endeavored equally to include all tornado occurrences and exclude all non-occurrences, his ratio of success is $\frac{28}{51+100-28}=.23$ (or 23 per cent.)

This I conceive to be the true measure of his achievement, so far as that achievement can be expressed by a ratio of verification. It is to be observed, however, that the ratio of verification falls far short of a just measure of success in scientific forecasting, for with the same skill in inference this ratio may be larger or smaller according as the phenomena foretold are normally frequent or rare. It is evident that Mr. Finley's effort and application of skill are the same, whether he regards his task as the prediction of the occurrence or of the non-occurrence of the tornadoes; but in the first case his ratio of verification is .23 and in the second it is .96. To pass from ratios of verification to comparative measures of success in prophetic inference, it is necessary to take account of relative difficulty.

If the forcaster were to make his p predictions at random it is probable that a certain number, e, of predictions would fortuitously coincide with occurrences. Making his predictions by the aid of inference, the number of coincidences is c. $c-e$ coincidences are thus the product of his skill in inference; and $c-e$ may be regarded as a measure of his success in inference, in precisely the same sense in which c has been regarded above as a measure of verification.*

Let i represent the ratio of success in inference. An analysis identical with that employed for the ratio of verification yields the equation:

* As the proof passes through my hands, I find the essence of this proposition somewhat differently stated in a letter by "G" in the number of *Science* for Aug. 15th. Speaking of tornado predictions he says: "The value of the expert work must be measured by the excess which is obtained over the man who knows nothing of the subject."

$$i=\frac{c-e}{o+p-c-e} \quad \cdot \cdot \cdot \cdot \quad (2).$$

In case of random prognostication the ratio of the fortuitous coincidences (e) to the number of predictions (p) is equal to the ratio of occurrences (o) to the total of cases—occurrences and non-occurrences—(s).

$$\frac{e}{p}=\frac{o}{s}, \text{ or } e=\frac{op}{s}.$$

Substituting this value of e in (2) and reducing, we obtain

$$i=\frac{c-e}{os+ps-cs-op} \quad \cdot \cdot \cdot \cdot \quad (3),$$

the desired ratio of success in forecasting by inference. Let us subject this formula to a critical examination.

Putting the value of i in the form $\frac{cs-op}{(o+p-c)s-op}$, it is evident that $i=1$ when $c=o+p-c$ or when $2c=o+p$. If $2c<o+p$, $i<1$; if $2c>o+p, i>1$. But c, the coincidence of $o+p$, can never exceed either of them and $2c$ cannot exceed their sum. Therefore i can never exceed unity.

Since c enters positively in the numerator and negatively in the denominator, the fraction increases rapidly with increase of c, and diminishes with decrease.

If $c=o$, $i=\frac{o(s-p)}{p(s-o)}$; since, on this assumption, o is the minimum value of p, $s-p$ cannot exceed $s-o$, and unity is the maximum value of i.

If $c=p$, $i=\frac{p(s-o)}{o(s-p)}$; when also unity is the maximum value of i.

If $c=\frac{op}{s}$, $i=0$: a mere return to the elements of the formula.

If $c=0$, $i=\frac{-op}{os+ps-op}$. Since neither o nor p can exceed s, op can exceed neither os nor ps. The denominator is therefore positive, and the value of i is negative. It is negative whenever $c<\frac{op}{s}$. If zero indicates failure of inference, a negative result should indicate perverse inference.

Since p cannot exceed s, it follows that $s-p$, the coefficient of o in the denominator, is always positive. The coefficient of o in the numerator is negative. The variations of o therefore affect i inversely. That is to say, the greater the number of occurrences, the less, *cæteris paribus*, the success in inference.

Similarly, the variations of p affect i inversely. That is to say, the greater the number of predictions the less, *cœteris paribus*, the success in inference.

If s is very large as compared to o and p, op may be neglected, and $i=\frac{c}{o+p-c}$, the ratio of verification. As s diminishes, the relative importance of op increases, and since the subtraction of the same quantity from both members of a proper fraction diminishes the fraction, diminution of s leads to diminution of i. i is therefore a direct function of s.

If $s=o$—that is, if all "cases" are "occurrences" all predictions are necessarily verified, or $c=p$. Then $i=0$. Inference can not aid in foretelling what invariably recurs.

If $s=p$—that is, if "occurrence" is predicted for all "cases"—all actual occurrences are verified, or $o=c$. Again, $i=0$. This is the case of the undiscriminating prediction of the non-occurrence of tornadoes.

It thus appears that i is a direct function of c and s, and an inverse function of o and p; but the ratio in which its value is affected by increments or decrements of the several variables is not the same for all. The coefficients of c, o and p have opposite signs in the numerator and denominator, while those of s are positive in both. This fact, and the further fact that the coefficients of s are relatively small, render the influence of its variations least of all.

Comparing c and o we see that in the numerator the coefficient (s) of c is never less, and usually greater, than the coefficient (p) of o—s being by definition the greatest possible value of p. In the denominator s is again the coefficient of c while $s-p$ is the coefficient of o. The fraction is therefore more sensitive to variations of c than to equal variations of o. With respect to c and p, no discrimination is possible, for they are symmetrically disposed in the fraction. The ratio of success in inference, therefore, varies directly and very rapidly with the number of coincidences between prediction and occurrence. It varies inversely and less rapidly with the number of occurrences and with the number of predictions. It varies directly, and still more slowly, with the total number of cases under consideration.

Reverting to the concrete illustration, if a greater number of Mr. Finley's predictions of tornado occurrences had been verified, his method and skill would be entitled to more credit. If the actual number of tornadoes had been greater, the chance of fortuitous

coincidence would have been increased, and the actual number of coincidences would indicate less skill. If the number of predictions had been increased, the chance of fortuitous coincidence would have been greater and the demand for skill less. Finally, if the total number of cases considered were increased, without any change in the other numbers, the chance of fortuitous coincidence would be diminished and greater skill would be implied.

Let us finally test the formula by inversion. The prediction of p occurrences of any phenomenon in a series of s cases or events implies the prediction of $p'=s-p$ non-occurrences. The ratio of success in the positive prediction being i, let us represent by i' the ratio of success in the implied negative prediction. The actual number of occurrences being o, the number of non-occurrences, o', evidently equals $s-o$. Representing by c' the coincidence of non-occurrence $(s-o)$ with non-prediction $(s-p)$, we have for its expression, all of s that is neither o nor p. If we write $c'=s-o-p$, we evidently err by twice, excluding that part of o which is also p, namely c; the true expression is $c'=s-o-p+c$.

If our formula for i (eq. 3) is general, it must include negative prediction, and we may write

$$i'=\frac{c's-o'p'}{o's+p's-c's-o'p'}.$$

Substituting the known values of $o'p'$, and c',

$$i'=\frac{(s-o-p+c)s-(s-o)(s-p)}{(s-o)s=(s-p)s-(s-o-p+c)s-(s-o)(s-p)}.$$

which, with expansion and combination of terms, becomes

$$i'=\frac{cs-op}{os+ps-cs-op} \quad - \; - \; - \; - \; (4).$$

The identity of the second members of (3) and (4) shows that the formula affords the same ratio of success whether the prediction is considered positively or negatively.

This is illustrated by applying it to the tornado predictions. Taking Mr. Finley's figures as cited above: $o=51$, $o'=2752$, $p=100$, $p'=2703$, $c=28$, $c'=2680$, $s=2803$.

$$i=\frac{(28\times2803)-(51\times100)}{(51\times2803)+(100\times2803)-(28\times2803)-(51\times100)}=.216.$$

$$i'=\frac{(2680\times2803)-(2752\times2703)}{(2752\times2803)+(2703\times2803)-(2680\times2803)-(2752\times2703)}=.216.$$

The satisfactory manner in which the formula responds to these several tests, warrants the believe that even if it is not the only criterion or the best criterion it is at least a useful criterion of success in prediction by means of inference. By its aid, it is certainly

possible to compare success in predicting tornadoes with success in predicting simple occurence of any other sort, so as to exhibit in a comparative way, the progress attained in the discussion of the several problems involved. But it is a condition of its application that the predictions in each case discriminate between two alternatives only. If the predictor attempt to determine for each day whether a certain stream will rise or not rise, his success can be compared with that of Mr. Finley, but if he attempts to determine for each day whether the stream will rise, fall, or remain stationary, and the selection of one alternative negatives the other two, his skill can not be weighed in this balance.

WASHINGTON, July 21, 1884. G. K. GILBERT.

TORNADO GENERATION.

A significant modification, in the theories underlying this subject, has recently been developed as new facts relating to the immediate environment of tornados have come to light. We find in the weather maps, which are so plentiful, an important addition to our knowledge of this environment. It has been the custom, till very recently, to study only the terrific effects of an individual tornado without determining the general conditions preceding it. In consequence, vague theories have sprung up in relation to the primary cause of a tornado, such as, enormous contrasts of temperature in masses of air, a meeting of cold northerly and warm southerly winds, cold air currents overrunning warm, etc.

The first weather maps of the Signal Service containing tornado tracks were published in 1875. These show the region of destructive action far to the southeast of the low center, which indeed had been announced by Redfield in 1846. The second set of maps were those of Mr. Finley published in 1881. These show conclusively the position of tornados to the southeast of the low center and in the region of gentle, uniform southerly winds, where there are no temperature contrasts greater than what might be expected from difference in latitude. The cooler northerly winds are found at some distance from the tornado and generally on the other side of the low center.

These principles were enunciated in an article in the *Washington Daily Post* for March 30, 1884, and have received confirmation

recently, by an article by Mr. Finley in *Science*, and by Mr. Davis in the August number of this journal.

Mr. Davis has given a combination, by superposition, of several maps containing tornado tracks, isobars and wind directions, on the principle elsewhere called "composite portraiture." A careful study will unravel the apparent tangle. It is a little doubtful whether anything can be gained, except space, by such a procedure. It is evident that if the principles set forth cannot be ascertained from each map, little can be obtained from such a maze which can only bewilder without giving the clear view which a single map presents. Moreover, if we take a storm center, with its isobars, etc., and place upon it another having a different latitude and longitude, it is evident that only the roughest approximate comparison can be made between isobars, isotherms and wind directions about them.

Attention is called to the fact that, in Chart I, the broken line separating south from west winds is drawn too far north and to the right of the low center; any wind directions from the west, on the east of a low center, can only be regarded as due to local causes and not entering into the general circulation about "low."

These charts show conclusively that tornados occur, as already described, in a region of uniform southerly winds to the southeast of "low." Mr. Davis, who has adopted the theory that tornados are to be found at the place of meeting of warm southerly and cool northwesterly winds, gives an original and rather remarkable explanation of the contradiction shown in these charts. He says, of tornados, this journal, p. 123: "their opportunity comes when a larger cyclonic circulation carries cool air over warm air, and thus produces the distinctly unstable atmospheric equilibrium necessary for the developement of violent local storms." Again on p. 125, "the most direct evidence of the rising of the cool northwesterly winds over the warm southerly is the rather abrupt disappearance of the former," also "standing at the center of a cyclone one would see that the wind coming from any point on the horizon, would rise as it approached the storm center and flow over the winds coming from points farther to the left.

This hypothesis is however subject to serious objections:

1st. Cool air, at the earth's surface, cannot overrun warm air, because, from its superior density, it will mingle with it or even *underrun* it. This in fact is in accordance with the theory held by some, namely: that an upsetting of the equilibrium in masses of air can only take place when cool air underruns warm.

2d. Tornado action is exceedingly local; while, if it were due to the overrunning of a more or less uniform northwest wind, we would naturally expect much more general action.

3d. The velocity of the northwest or west wind seldom exceeds 10 or 15 miles per hour, at the times under discussion, while these local storms frequently are preceded by a hurricane 70 and 80 miles per honr.

4th. The abrupt disappearance of the west wind is imaginary. If we draw radial lines from a "low," on a large number of maps having tornado action, and take upon these lines mean velocities and directions of the wind at distances of 100, 200, etc. miles from "low," eliminating or allowing for local effects, we shall find uniform velocities and the directions of the wind following the law laid down by Buys Ballot relative to air circulation about "low." Westerly winds naturally would be cooler than southerly, but the temperature of these winds will be found lower and lower as we proceed around toward the west and north.

5th. The supposition, that westerly winds rise above southerly due to their spiral course toward and about "low," is not tenable; for, if the wind blew in a *radial* course toward "low," there would be no doubt but that the air would rise uniformly along the whole interior advancing circle of wind, provided the velocity were uniform on all sides. Now if we regard a simple change in the direction from radial to spiral, the air would still have a tendency to rise uniformly, or if there were overlapping, it would be exceedingly slight and only at adjacent layers.

6th. Considering the commonly accepted theory, that, at a tornado centre there is a powerful updraft, a northwesterly wind would be carried still farther up, when it reached the scene of action, instead of descending to the earth as a hurricane.

7th. It would be singular, if there were any such overflow over a large region, if it did not from time to time dip down and give us an occasional west wind instead of the uniform southerly current given by observation at all places save at those where there is thunder-storm action.

The error in this theory is a fundamental one. H. F. Blanford has shown that the meeting of two air currents can by no possibility produce a wind velocity greater than either of them. That there is some occult force generated is extremely improbable.

Another error in views of tornado generation seems to be an assumption that there is a forcible uprush in its centre due to a

partial vacuum. Barometric observations during the passage of thunder-storms, "infant tornados," indicate a rise in pressure. This question of barometric observations in the center of a tornado is a very important one.

It is to be hoped that no one who has a barometer will be deterred from its use in a tornado, by Prof. Paul's somewhat poetical and imaginary description of an enthusiastic meteorologist flying on the summit of a tornado reading and recording his barometer as he flies. See this journal for August. The barometer may be carried to the cellar or dug-out and read every three minutes until the tornado has passed. In case the barometer is an aneroid, the only precaution needed is to gently tap on the glass face two or three times, with the edge of the finger nail, just before reading. These observations should be made, not only at the time of tornado action, but also at the approach of a heavy thunder-storm. If there could be concert of action, by all having barometers in tornado districts, for two or three years, evidence of a convincing character would be obtained, which would enable us to verify or disprove the present partial vacuum theories.

The carrying of shingles and heavier objects into the air may be due to a straight wind, as we frequently notice clouds of dust carried upward 200 or 300 feet in front of a gale preceding a thunder-storm.

It is believed that the time has hardly come for establishing a complete theory of tornado generation. We can do little beside determining what it is not, at present. A careful charting and study of general conditions existing during thunder-storms and electrical action in the vicinity of the more violent outbursts seem to be a promising line of inquiry.

A typical thunder-storm, as has repeatedly been observed, is attended by the following phenomena: Toward a region of gentle, southerly, heated air currents, there advances, suddenly and nearly always from the west, a dark cloud with electrical manifestations; as this approaches the observer on land, there is seen at the earth's surface an immense cloud of dust rising to hundreds of feet in altitude. Immediately the gentle southerly wind gives place to a violent westerly hurricane frequently rising to 80 and more miles per hour, as determined by actual anenometric observations. The barometer which has been steady all day or slowly falling, has a downward course for a few minutes, when it suddenly rises one or two millimetres and in a few minutes falls again. The electrical

appearances become very intense, passing quickly to the east, followed by light winds and sporadic lightning and thunder.

The whole appearance is as if a disturbance, having its own source of energy, was carried forward by some force, and not as though it were continually being recuperated by an upsetting of the equilibrium in the air in its front. This view is strengthened when we learn that the same powerful action has passed over the country for hundreds of miles to the east.

Aug. 20, 1884. H. A. HAZEN.

RECENT DISCOVERY OF THE TRUE SOURCE OF THE MISSISSIPPI RIVER.

By CAPTAIN WILLARD GLAZIER.

CHAPTER I.

THE OLD EXPLORERS.

While crossing the continent on horseback from ocean to ocean in 1876, I came to a bridge which spans the Mississippi between Rock Island, Illinois and Davenport, Iowa. As I saw the flood of this mighty stream rolling beneath, I turned in imagination to its discovery in 1541; I saw the renowned De Soto on its banks and buried in its depths; I accompanied Marquette from the mouth of the Wisconsin to the mouth of the Arkansas; I followed Father Hennepin northward to St. Anthony's Falls, and saw the daring La Salle plant the banner of France on the shores of the Gulf of Mexico.

Musing thus upon the exploits of the heroic old explorers who led the way to this grand and peerless river of North America, I felt that it was a subject of much regret, that, although its mouth was discovered by the Chevalier La Salle nearly two hundred years ago, there was still much uncertainty as to its true source. Within the last century several expeditions have attempted to find the primal reservoir of the Great River; Beltrami, Nicollett and Schoolcraft have each in turn claimed the goal of their explorations. Numerous lakes, ponds and rivers have from time to time enjoyed the honor of standing at the head of the Father of Waters.

Schoolcraft finally located a lake which he named Itasca, as the fountain head, in 1832, and succeeded in securing for it the recognition of geographers and map makers. Notwithstanding the fact however, that the new claim for geographical honors was very

generally accepted as the source, I had frequently been told that many Indians denied that their ideal river had its origin in Lake Itasca, but that there were other lakes and rivers above and beyond that lake. These reflections led me to conclude that there was yet a rich field for exploration in the wilds of Minnesota.

Want of means and a combination of unfavorable circumstances prevented for several years the accomplishment of my purpose to penetrate to the true source of the Mississippi. The month of May, 1881, found me sojourning a few days at Cleveland, Ohio, where I had halted in my journey westward from New York. On the first day of June I proceeded to Chicago, and from thence to St. Paul, Minnesota, where I was joined by my brother George, and Barrett Channing Paine, of Indianapolis. The month of June was spent at St. Paul in preparation. Tents, blankets, guns, ammunition, fishing-tackle, and other equipage necessary to a six weeks' campaign in the wilderness, were provided for the little band which was to form my expedition.

Having completed arrangements, I left St. Paul on the morning of July fourth, with Brainerd as my immediate objective. Short halts were made at Minneapolis, Monticello, St. Cloud and Little Falls, on our way up the river. Brainerd was reached July seventh. This enterprising little village is situated at the point where the Northern Pacific Railway crosses the Mississippi; is near the boundary of the Chippewa Indian Reservation, and is the nearest place of consequence to Lake Itasca. Here I again halted to further inform myself concerning the topography of the country; to decide upon the most feasible route to my destination, and to provide such extra supplies of rations and clothing as might be considered essential to the success of our enterprise. After consulting my maps, I concluded that, while Schoolcraft and others had found Itasca by going up the river through Lakes Winnibegoshish, Cass and Bemidji, the most direct course would be by way of Leach Lake and the Kabekanka River.

CHAPTER II.

THROUGH THE CHIPPEWA COUNTRY.

A careful study of the route to Leech Lake, with a few valuable suggestions from Warren Leland, an old resident of Brainerd, led me to seek wagon conveyance to the former place over what is known in northern Minnesota as the Government Road. This road stretches

for seventy miles through trackless pine forests and almost impenetrable underbrush; and the only habitations to be seen from it are the half-way houses, erected for the accommodation of teamsters who are engaged in hauling government supplies; and the occasional wigwams of wandering Indians. It was opened in 1856, by James Macaboy, for the convenience of Indian agents and fur traders.

Fully equipped and with a driver celebrated for his knowledge of the frontier, we commenced at eight o'clock on the morning of July twelfth, our wagon journey to Leech Lake, the third objective in my expedition to the head waters of the Mississippi. John Monahan who held the reins in this seventy-five mile journey over one of the roughest roads of Minnesota, is a true son of Erin, who need not take a back seat for Hank Monk, or any of the famous drivers of the border.

A ride of between three and four hours brought our little party to Gull Lake, where a halt was made for rest and refreshments.

Gull Lake was for many years the home and head quarters of the noted Chippewa Chief, Hole-in-the-day, and was the scene of many sanguinary struggles between his braves and those of the equally celebrated Sioux chief, Little Crow. The remnant of a block house, fragments of wigwams, and a few scattered graves, are all that is now left to tell the tale of its aboriginal conflicts.

A family of four persons, domiciled in a log house, constitute the entire white population of the place. Reuben Grey, the genial patriarch who presides over the solitary household in the wilderness, delights in the title of landlord, and his hotel has become somewhat famous as one of the pioneer half-way houses between Brainerd and Leech Lake.

My arrival at Gull Lake was duly celebrated by launching a canoe, which soon returned with a fine mess of fish. These, with such fruits and vegetables as were in season, afforded a dinner which our appetites whetted by a forenoon's jolting in a country wagon, had fully prepared us to enjoy.

After dinner we resumed our journey, with Pine River as the evening destination. Sometimes in the road, sometimes out of it; now driving along the shore of a lake, and again over huge logs and boulders, it was voted that our ride to Pine River was unlike anything we had ever elsewhere encountered.

The ranche of George Barclay, the only white habitation between Gull Lake and Leech Lake, was reached at five o'clock in the even-

ing. Here we were most agreeably surprised to find very good accommodations for both man and beast. Barclay is a decided favorite with the Indians, and his prosperity in this isolated corner of Minnesota is largly due to his friendly relations with them. He is always supplied with guns, knives, beads, tobacco, and such other goods as are in demand by his dusky neighbors, for which he receives in exchange furs, game, snake-root, and any other products of the forest, which find a ready market at Brainerd or St. Paul.

Much valuable information was obtained at Pine River concerning our route to Leech Lake and beyond, the peculiar traits and characteristics of the Indians whom we were likely to encounter, and those persons at the agency who could be of most service to us.

An excellent breakfast on the following morning, with the prospect of reaching the first grand objective in our line of march, put my little party in the most exhuberant spirits for the day; and nothing but jolting over one of the most indifferent and rugged roads I have ever encountered, could have lessened the enjoyment of our journey. A short halt was made for lunch at Fourteen Mile Lake. This was our first meal in the open air, and enabled us to gauge more accurately our supply of rations. It was readily discovered that we should need at least a third more provisions per diem for our expedition than would be required for the ordinary occupations of indoor life; and I at once decided to provide an additional supply of bacon and dried meats before passing Leech Lake.

After lunch my brother and Mr. Paine took a bath in the lake, while I found amusement in duck-shooting and chatting with some straggling Chippewas, who were about launching their canoes for a six weeks hunting and fishing excursion. These were the first birch bark canoes I had seen, and were regarded with considerable interest, as they were indispensable to the success of my undertaking. Curiosity led me to step into one of the canoes, when from want of experience I was precipitated into the lake, much to my own discomfort and chagrin, and the amusement of the Indians. Being unable to swim, I was congratulated upon a capsize in shallow water. Finally, resolved upon more caution in the future, I continued my journey towards Leech Lake, which was reached at four o'clock in the afternoon.

[*To be continued.*]

SOLAR HEAT AND TERRESTRIAL DILATABILITY.

By Sr. F. D. Covarrubias.*

INTRODUCTION.

It is not without a profound sentiment of dread that we dare to publish the result of our researches upon the influence that solar heat exercises upon the general figure of the globe. Though this action, like that of heat from all other sources, produces necessarily upon matter the phenomenon of dilatation, it is none the less true that no one notices the terrestrial dilatation, and that even in the most extended and delicate geometrical operations, like those of geodesy, it has always been tacitly or expressly admitted that the variations of temperature do not influence in a sensible manner the value of magnitudes measured at the surface of the earth.

Dilatability has been, nevertheless, recognized as one of the general properties of matter, and its effects have been measured in most of the simple and composite substances which compose the terrestrial strata accessible to man. It is not possible to understand how a property can be denied to the whole which is admitted in the parts; and in seeing such an exclusion which nothing seems to authorize, it might be said that, according to the metaphysical ideas of the ancient alchemists, a state of isolation or of chemical purity of a substance was believed necessary to give it the property of dilatation.

It is true that the coëfficients of the dilatation of solids are always very small, and that, in most cases, we have the right to ignore very small effects; but, in cases where we must consider the whole earth or large portions of its surface, we believe that it is no longer permissible to set aside, as insensible, the expansive effects of heat, at least, without having made an attentive examination of all the phenomena which it is said to produce and without having tried to measure it.

It is precisely in comparing among themselves the results of the best geödetic works which have been made to determine the figure and dimensions of the earth that for the first time we thought to perceive the influence of the temperature for each of the places in which these great operations were performed; and now, in regarding these same temperatures, we might perhaps obtain a more perfect accord between the results obtained by geodesy, and those which have been furnished by numerous measurements of the length of the pendulum beating seconds, which, as we know, gives to the earth a flattening which is much greater than that deduced by the geödetic operations. We will see farther on the result of these researches, and though it may not be decisive—since to obtain it we were obliged, for want of exact data, to employ approximate numbers—it suffices, in our opinion, to demonstrate that the influence of solar heat is far from being unimportant in this kind of work, and to attract the attention of scholars so that they will direct their efforts toward the exact measurement of that influence and all the phenomena which depend upon it.

* Translated from the *Anales del Ministerio de Fomentó de la Republica Mexicana*, vol VII. The original is in French, and the author apologizes for any solecisms due to the use of a language not his own. We have thought the ideas of this memoir sufficiently novel and fertile to be worthy of a reproduction—the more so as the most of our readers would not expect to find matter of this kind in the *Anales* which are devoted largely to agriculture and mining. It may interest the reader to know that Sr. Covarrubias has recently been intrusted by his government with a scientific mission abroad.

Another fact, which we remarked not long ago, made us reflect anew upon the effects that solar heat seems to exercise upon the superficial strata of the earth. At the time of our residence in Central America as Envoy Extraordinary and Minister Plenipotentiary from Mexico, we made some astronomical observations to determine the geographical position of the city of Guatemala, and, during the course of this work, we had occasion to notice quite frequently the slight movements of the soil, which we supposed must be attributed to the volcanoes which abound in that country. In fact, the tremblings of the earth which we observed there—as much trepidation as oscillation—lasted for a greater or less time, but were always very appreciable; while the movements of which we speak were instantaneous, and not noticeable without the help of instruments. They seemed to us to be comparable to those which were produced by a blow applied to the mass of brick which sustained our altazimuth, or like the shocks which would be felt by a body moving slightly under the action of an intermittent force. In regarding the moon, we could see the reticule become displaced upon the bright limb, and make instantaneous vibrations of which the size was certainly greater than 15" of arc.

Sometimes they succeeded each other without interruption, and produced upon us the impression of the sudden shocks which the molecules of a body, which is but slightly dilatable, would experience when submitted to changes of temperature. From this time we decided to study, from the point of view of the action of solar heat, the phenomena which could depend on it, and of which the usual explanations did not seem entirely satisfactory. It is perhaps the only method of verifying the effects of a hypothetical cause so difficult to demonstrate directly.

Our work is far from being complete, not only because only five or six months have passed since it was undertaken, but also for lack of data sufficient to establish an entirely new theory which, up to a certain point, is in opposition to the ideas generally received. These motives are, without doubt, sufficient to justify our hesitation, our dread, and even the natural distrust of our own ideas, that we feel on submitting the following pages to the examination of scientific men.

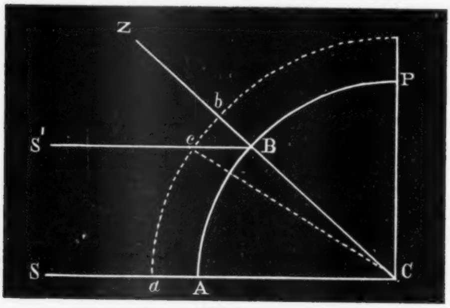
We beg them, in so doing, to consider them only as an appeal which we dare address to physicists, to astronomers, to geodesists and to geologists who wish to study from this new view of the influence of solar heat the different phenomena which form the object of their respective specialties. They will thus find new means which have, without doubt, escaped us, let it be to re-inforce, to correct, or to overturn our theory; but we beg them, at the same time, not to forget that, whatever the difficulties which may be met in the explanation of certain facts by this new theory, there remains always the difficulty no less considerable of explaining why the earth, of which some parts are submitted to an enormous heat by the action of the sun, is without dilatability, that is, without one of the general properties of matter.

The plan which we have followed in the writing of this memoir consists in establishing theoretically the law of the decrease of the dilatation in the terrestrial ellipsoid, by reason of the differences in heat of its various parts, and, then, in comparing the results of the theory with those of the observation of some facts of which the production may, it seems to us, be traced to this cause. If it has been impossible for us to avoid entirely the use of analysis, especially in the first section, we have represented these results by means of figures, so that our readers will be in a condition to appreciate more readily the *ensemble* of our little essay, almost without the help of the tiresome language of formulæ.

I. Establishment of the Theory.

To establish the general law of the distribution of solar heat upon the earth's surface, we may suppose that the sun is always upon the equator and that its rays fall upon the earth parallel to this plane, and consequently with an obliquity increasing with the latitude of each place of observation. Although this supposition is not rigorously true when we consider only the effects of solar action in a definite period less than a year, it is, on the contrary, practically exact when we deduct the variations of temperature produced by the different positions of the sun, and occupy ourselves only with the mean heat which any point of the earth receives in the course of an entire year, during which the mean portion of the sun is really the equatorial plane.

Let, then, S (fig. 1) be the sun upon the plane A C of the equator. Its rays will fall vertically upon A; but upon any other point B, of which the latitude is l=A C B, they will form with its vertical B Z the angle S' B Z which is precisely equal to x, and, moreover, they will have to traverse the atmosphere obliquely, in depth B c greater than its height A a or B b. For this double reason the intensity of the caloric rays, in arriving at B, will be less than at the point A of the equator. Designating by I the intensity of the solar heat at A, by i that which it has at B, by m the vertical height of the atmosphere and by e its oblique depth Bc, we will have:



(FIG. 1.)

$$i = \frac{m \cos. l}{e} I$$

since uopn the point B the vertical effect of I is only I cos. l, and that in traversing obliquely the atmosphere it loses as much more as the depth e is greater than the height m. Taking this last quantity for unity to measure the value of e, we can write:

$$\frac{i}{I} = \frac{\cos. l}{e} \quad \text{- - - - - - - -} \quad (1)$$

This formula shows that the solar heat upon the earth is directly proportional to the atmospheric depth which the rays must traverse.

If the terrestrial globe were exactly spherical, then (since the dilatation is proportional to the temperature which produces it) the ratio (1) would give the value of the dilatation at latitude x, that of the equator being 1; and it would determine, at the same time, the law of the variations which the spherical figure must experience under the influence of heat; but to find these same modifications of figure upon an ellipsoid, we believe that we should first calculate the ratio which should exist between the elongation of the radius at the latitude l and that of the equatorial radius, and then multiply it by the value of $\frac{i}{I}$.

For this purpose, a and b being respectively the equatorial and polar radii of the earth, we have according to Besseli:

$$a=6,\ 377397^{m} \qquad b=6,\ 356079^{m}$$

from which there results for the terrestrial flattening

$$k=\frac{a-b}{a}=0,\ 003343 \text{ or about } \tfrac{1}{299}.$$

Although these numbers have been obtained by the discussion of geodetic measurements, and in consequence influenced by the terrestrial dilatation of which no notice has been taken, they will serve, nevertheless, to calculate this effect of heat, since it must be small compared to the dimensions of the earth.

Since according to equation (1) the pole receives no heat from the sun, we will suppose b constant, and will search for the variation which the flattening experiences on account of a supposed elongation in the equatorial radius. The preceding expression gives by differentiation:

$$dk=(1-k)\frac{da}{a} \quad - \ - \ - \ - \ - \ - \ - \quad (2)$$

and as the expression of the terrestrial radius at the latitude x, of which the value depends at the same time upon a and upon k, is, with exactitude sufficient for our object, $r=a\ (1-k \sin.^2 l)$, we will have:

$$dr=(1-k \sin.^2 l)\ da-a \sin.^2 l dk.$$

But since the value of da gives $adk=(1-k)\ da$, this becomes $dr=(1-k \sin.^2 l-(1-k) \sin.^2 l)\ da$, and by reduction:

$$\frac{dr}{da}=\cos.^2 l.$$

Such is, in general, the ratio which exists between the elongations of any radius of the ellipsoid and that of its equator, the figure of the solid remaining always ellipsoidal; but, since by our hypothesis, these elongations should be produced according to the law of heat represented by ratio (1), we obtain the ratio thus modified, by multiplying this last ratio by the preceding, and we have :

$$\frac{dr}{da}=\frac{\cos.^3 x}{e} \quad - \ - \ - \ - \ - \ - \ - \ - \ - \quad (3)$$

This new ratio expresses the law of the intensity with which the dilatation of the superficial strata of the earth should be produced, and it shows that, if this phenomenon is appreciable, the general figure of the globe must differ a little from that of an ellipsoid of revolution.

To calculate the oblique depth, e, of the atmosphere, its vertical height being always taken for unity, we will now call m the terrestrial radius expressed in the same unit, and suppose the two curves which limit the atmosphere and the earth in a section made following the plane of a meridian, as in fig. 1, to be circular and concentric.

If we take the vertical line C Z for the axis of ordinates, we can take the depth $Bc=e$ as forming part of a straight line, making the angle l with that axis, and meeting it at the distance m from the origin.

Its equation will be then :

$$y=l \cot.\ x+m.$$

That of the circle which limits the atmosphere being $x^2+y^2=(m+1)^2$, the elimination of y between these equations will give for the abscissa of the point c of the intersection of two lines:

$$x=-m \sin.\ l \cos.\ l \pm \sin.\ l \sqrt{(m+1)^2-m^2 \sin.^2 l};$$

but we have $x=e \sin.\ l$, and thus (taking the positive value of the radical):

$$e=\sqrt{(m+1)^2-m^2 \sin.^2 l}-m \cos.\ l.$$

Let us make:

$$\sin. w = \frac{m}{m+1} \sin. l \quad - \ - \ - \ - \ - \ - \quad (4)$$

and we will have finally:

$$e = (m+1) \cos. w - m \cos. l.$$

It should be observed that the auxiliary angle w is that which e makes with the straight line which from its upper extremity is directed to the center of the earth; and that $l-w$ is the angle formed by the two radii which terminate at the extremities of e.

This value of e has the inconvenience of being expressed by the difference of two large numbers; but if we introduce the values of m or of $m+1$ taken from equation (4), it is transformed into the following form, which is more convenient for application:

$$e = (m+1) \frac{(\sin. \ (l-w)}{\sin. \ l} = m \frac{\sin. \ (l-w)}{\sin. \ w} \quad - \ - \ - \ - \quad (5)$$

It is by means of the formulas (4) and (5) that we have calculated the following table for each 5° of latitude for the whole quadrant. We have supposed 45 miles to be the height of the atmosphere, at least of that part which is active as an absorbent of heat and of light, which amounts to making $m=100$. The table also contains the values of the functions $\frac{1}{e} \cos. l$ and $\frac{1}{e} \cos.^3 l$, which, according to our theory, express respectively the laws of the diminution of solar heat and of the dilatation upon the surface of the earth.

x	w	e	$\frac{1}{e} \cos. l$	$\frac{1}{e} \cos.^3 l$
0°	00° 00′ 00″	1.000	1.0000	1.0000
5	4 57 1	1.006	0.9906	0.9831
10	9 54 00	1.015	0.9701	0.9409
15	14 50 53	1.035	0.9334	0.8708
20	19 47 37	1.064	0.8834	0.7800
25	24 44 9	1.102	0.8225	0.6756
30	29 40 23	1.153	0.7513	0.5635
35	34 36 13	1.216	0.6724	0.4512
40	39 31 32	1.301	0.5888	0.3455
45	44 26 8	1.407	0.5025	0.2513
50	49 19 43	1.545	0.4161	0.1719
55	54 11 52	1.726	0.3323	0.1093
60	59 1 54	1,971	0.2537	0.0634
65	63 48 36	2,314	0.1826	0.0326
70	68 29 44	2.822	0.1212	0.0142
75	73 00 40	3,629	0.0713	0.0048
80	77 10 34	5,053	0.0344	0.0010
85	80 30 57	7,927	0.0110	0.0001
90	81 55 50	14,177	0.0000	0.0000

Observe that when l is not very large, the value of e differs little from $\frac{1}{\cos.\ l}$. In fact, the triangle Bc C gives:

$$\tan.\ l = \frac{(m+1)\ \sin.\ (l-w)}{m-(m+1)\cos.\ (l-w)}$$

but $l-w$ being always small, since its value does not surpass 2°, even for $l=70°$, we can employ, instead of its cosine, the series $\cos.\ (l-w)=1-\frac{1}{2}(l-w)^2$, and we will have $(m+1)\ \sin.\ (l-w)=\tan.\ l-\frac{1}{2}(m+1)(l-w)^2\ \tan.\ l$. In the second member of this equation we may make use of the approximate value $l-w=\frac{\tan.\ w}{m+1}$ to obtain:

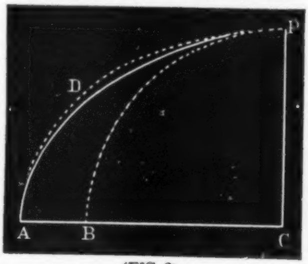
$$(m+1)\sin.\ (l-w)=\tan.\ l-\frac{\tan.^3\ l}{2(m+1)}$$

Eliminating the first member between this equation and the first value (5) we find:

$$e=\frac{1}{\cos.\ l}-\frac{\tan.^2\ l}{2(m+1)\ \cos.\ l}$$

It is evident that, $m+1$ being a large number, we may take $e\ \cos.\ l=1$ when w is not greater than 60° or 70°; so that for the torrid and temperate zones our two functions $\frac{1}{e}\cos.\ l$ and $\frac{1}{e}\cos.^3 l$ could be replaced respectively by the more simple $\cos.^2 l$ and $\cos.^4 l$. We retain, however, the first values.

Up to the present we can not count upon sufficient data to determine the absolute height of the equatorial swelling produced by dilatation, and, consequently, it is impossible for us to estimate that which corresponds in the other latitudes according to the law $\frac{1}{e}\cos.^3 l$; but this ratio can give us an idea of the relative curvature at each point of the meridian, although with the inevitable imperfection due to the lack of data, and also from the probable smallness of the swelling itself as compared with the radii of the earth. For this purpose, upon a slightly eccentric ellipse, or even upon a circular quadrant B P (fig. 2), let us take the prolongation of the radii differently inclined upon A C, which represents the equatorial radius, the lengths proportional to $\frac{1}{e}\cos.^3 l$. The curve which will pass through the extremities of these straight lines will represent the terrestrial meridian, as it results from the superficial dilatation of the globe according to our theory.



(FIG. 2)

It may be seen in this figure that the general curvature of the meridian decreases slowly in the low latitudes; but that toward the third (D) of the quadrant, and a little beyond it it becomes less variable, and produces a kind of relative depression or flattening as compared with the curvature, which would correspond to an ellipse with the same axes. This is indicated by a line of points. We will see, farther on, that this fact seems to be in accord with several physical phenomena.

In order to analyze more easily the function $\frac{1}{e}\cos.^3 l$, and also with the intention of rendering more simple its comparison with the observed facts, we will con-

sider it as the equation of a curve referred to two rectangular axes. We put then:

$$y=\frac{1}{e}\cos.^3 l$$

an equation which, with the value of e, will become:

$$y=\frac{\sin. l \cos.^3 l}{(m+1)\sin. (l-w)}$$

The angle w being given by the formula:

$$\sin. w=\frac{m}{m+1}\sin. l$$

we have by differentiation:

$$\frac{dw}{dx}=\frac{m\cos. l}{(m+1)\cos. w}=\tan. w\cot. l$$

With this value we will obtain by the differentiation of our function, by means of transformations which it is not difficult to comprehend:

$$\frac{dy}{dx}=-(3\tan. l+\tan. w)y$$

$$\frac{d^2y}{d l^2}=\left\{(3\tan. l+\tan. w)^2-\frac{3}{\cos.^2 l}-\frac{\tan. w\cot. l}{\cos.^2 w}\right\}y$$

with the aid of the value of $\frac{dw}{dx}$ we can give to these two differential coëfficients a form which seems to us more simple for this application. Representing by a the constant $\frac{m}{m+1}$ we have

$$\tan. w=a\frac{\cos. l}{\cos. w}\tan. l$$

and the ratio of these cosines can, in its turn, be expressed as a function of e and of m. In effect, in the triangle formed by e, m and $m+1$, the last two sides have respectively w and $180°-l$ for opposite angles; and, in consequence, we will have $m^2=e^2+(m+1)^2-2e(m+1)\cos. w$, and $(m+1)^2=e^2+m^2+2em\cos. l$ from which it results:

$$\frac{\cos. l}{\cos. w}=\frac{1}{a}\,\frac{2m+1-e^2}{2m+1+e^2}$$

and, representing by n this ratio, we have $\tan. w=an\tan. l$, which gives to the differential coëfficients the form:

$$\frac{dy}{dx}=-(3+an)y\tan. l$$

$$\frac{d^2y}{dx^2}=\left\{(3+an)^2\tan.^2 l-\frac{3+an^2}{\cos.^2 l}\right\}y$$

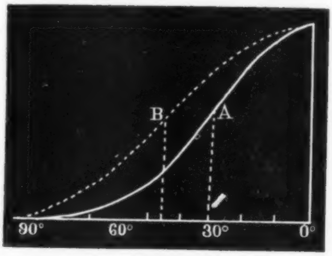
The first becomes null for $l=0°$ and for $l=90°$, points which, in consequence, correspond respectively to the maximum and minimum ordinates, and indicate the horizontal tangents. The second becomes also null for $l=90°$, expressing an inflection in this point. The other inflection corresponds to the value of l given by the disappearance of the factor of y, which is:

$$\sin. l=\frac{\sqrt{3+an^2}}{3+an}.$$

The value of n depends upon that of e^2, which is, in its turn, a function of the unknown l; but with the aid of the table of the values of e, the preceding equation may be calculated easily and rapidly by means of successive approximations. In supposing $l=30°$ or $e=1.15$, we find that the inflection takes place in the

point which corresponds to $l=30° 1' 40''$, and a second approximation with this new datum leads to the same result.

For the use of persons unaccustomed to algebraic language, we represent this curve in the 3d figure, the latitudes being counted upon the horizontal axis; while in figure 2 they were counted upon the arc of a circle, and the corresponding ordinates upon the prolongations of these radii, We see in A the point of inflection corresponding to the latitude of 30° 1' 40'', and at the maximum divergence, as we have pointed out in figure (2), between our curve and the ellipse.



(FIG. 3)

Analyzing in a similar way the function $y=\frac{1}{e}\cos. l$, which represents the law of the degradation of solar head upon the earth, we find that it has also an inflection for $l=45° 00' 20''$. In the same figure 3 this curve is represented by a line of points, of which B is that of inflection. It is toward the same latitude of 45° that the greatest divergence of the two curves took place, and we will see by the result the consequences of this indication of the theory.

The lines which precede, seem to us sufficient for the exposition of our theorical ideas, concerning the law of the dilatation of the surface of the globe. We will pass now to the comparison of this law with some physical phenomena of which the production seems to us to be connected with the same doctrine.

(To be continued.)

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—EDITED BY—

M. W. HARRINGTON,

Director of the Astronomical Observatory, Ann Arbor, Michigan.

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The editor solicits communications on all subjects having any bearing on Meteorology. Correspondence on events of current interest, with the accounts of local newspapers concerning them, and all publications relating to American Meteorology will be welcome. Address all editorial communications to MR. HARRINGTON; all others to W. H. BURR & Co.

LITERARY NOTES.

—M. Jamin proposes in the *Comptes rendus*, (and Mr. Symons republishes the proposal in his magazine), that the relative humidity of the tables be replaced by what he calls hygrometric richness. That is, if f and F are the vapor tensions for the temperature, and for saturation at that temperature, and H the total atmospheric pressure, he would replace $\frac{f}{F}$, by $\frac{f}{H-f}$. This is done because, while the latter is not more difficult to compute than the former, it states a simple ratio of the moisture and is more variable than the other.

We may note that the proposal is not altogether new. The hygrometric quality, $\frac{f}{H}$, has been long known and is explained in elementary textbooks, as Stewart's Elementary Treatise on Heat. Doubtless, either the hygrometric richness or quality would be a proper addition to our tables, but they can hardly replace relative humidity wich expresses the drying properties of air and the probability of dew or rain.

—In a notice of Dr. Ames' excellent report of the relief expedition for the Ohio flood sufferers we refer to the "preceding." When we wrote the "preceding" was a note on Vance's report, which was afterwards placed on p. 150, and the reference to it as preceding was overlooked. Prof. Mendenhall's reports are altogether excellent, and we had no intention of speaking slightingly of them.

—The *Washburn Observatory*, at Madison publishes a list of 608 auroras observed at Sauk City, Wisconsin, by Mr. F. G. T. Lueders, for the 24 years ending 1883. In the list are included (without numbers) 62 luminous appearances seen in or near the magnetic meridian but not positively identified as auroras. The author has observed the frequent occurof rence auroras and heat-lightning together and notes it for each case in his list.

—We are much pleased to see that the *Pilot Chart* of the Hydrographic Office is continued. The August chart has reached us as promptly as usual. The ice to the southeast of Newfoundland still shows unusual southward extension. It was found in July as far south as latitude 41°. A field of ice was seen during the month about 200 miles east of Boston. Of the half dozen waterspouts charted, one was at the mouth of Chesapeake Bay and two off the mouth of the Mississippi River. The path of the wrecked schooner Maggie M. Rivers is given in seven positions from January 17 to June 26. The path is a zigzag one, but is, on the whole, away from the coast until May 9, then toward the coast. This is off Cape Hatteras, not far (about 40 miles) from which the wreck occurred. The distance on May 9 was upwards of 500 miles, a little south of east. On June 26 she had returned about half this distance, and was about east southeast from the cape. We fail to find in her path unmistakeable signs of the drift of the Gulf Stream.

—In the *Kansas City Review* for August Lieut. Parker favors the formation of a corps of Arctic explorers consisting of Innuits educated and trained for the purpose, led by some competent white naval officer. He thinks the Innuits to be educated should be taken from home at the age of eighteen or twenty. The idea is a novel one, and has many things in its favor. It is doubtful, however, if the Innuits could be trained to meet the demands of such scientific work. Their intellectuality is not remarkably high, and, like other uncivilized men, at the age of maturity their minds grow rigid, after which time they do not learn easily. The proverb that you can not teach an old dog new tricks applies as well to uncivilized man as to the lower animals, and the phenomenon of mental rigidity is well known to those who have lived among savages or even races so high in the scale, like the Innuits, that a better name belongs to them. Continued capacity for learning is the exclusive heritage of the descendants of studious ancestors through many generations.

—According to the *Alabama Weather Report*, the month of June was unusually wet. For about a sixth of the State the rainfall was upwards of 8 inches and for half of the State it was between 6 and 8 inches. Thunderstorms were of almost daily occurrence.

—The *Illinois Crop Report*, No. 114, issued by S. D. Fisher, gives the weather report for June. Thunder storms were numerous and averaged one for every six days in the northern counties, one for each five in the central, and one for each three in the southern counties, thus showing an increased frequency from the north southward.

—Professor J. T. Lovewell publishes monthly in the *Kansas City Review* the meteorological means for Topeka arranged in decades, with some general notes as to the weather. This journal contains much meteorological matter; often of important character. In the August number, however, we regret to note that the article of Isaac P. Noyes on meteorological discoveries is imperfect and misleading. It is a sort of apotheosis of the weather-map, but the author seems to us to have given his readers a very incomplete account of the weather relations of high and low centres of pressure, thus failing in calling attention to the principal attractions of his divinity. Moreover, he has entirely misapprehended the relations of local storms to the general cyclone. The former do not generally follow the latter, but are in its southeast quadrant. We may also doubt if it is fair to the reading public to intimate to it that the centres of high and low pressure are, so to speak, atmospheric satellites, following their course completely around the earth again and again. The fact is that we frequently see, to all appearances at least, the beginning and the end of storm-areas.

—The *Notes, Queries and Answers* published monthly by S. C. & L. M. Gould at Manchester, N. H., is a curious and interesting journal in which any one can find something to entertain, instruct, or amuse him. The editors call especially for weather proverbs, and, as they are well worth discussing and sifting, we recommend our readers to send the collections they have made, since Lieut. Dunwoody's list appeared, to the publishers of the *Notes*. The following, which we clip from it, are new and have decided meteorological bearings:

A PHENOMENON. Near Black River Falls, in Wisconsin, is a spring situated on the bank of a small mill-stream, which does not freeze over till late in the season. The spring is about three feet deep and curbed up with wood to the top of the ground, and the water stands level with the top of the ground. No ice has appeared on it yet this winter (now January 29), though the mill pond has been frozen since November, and much of the time since then the thermometer has ranged from 10° to 28° below zero. Later in the season the spring will freeze very hard so that one will be obliged to cut the ice with an ax every time to get water; and it will continue to freeze late in the spring, when the ice is softening and going out of the mill pond. Explain the reason of this phenomenon. OXFORD.

The following extract from a private letter to a resident of the village of Malone, N. Y., from a well known citizen of Waterbury, Conn., detailing a personal examination of that natural curiosity, the frozen well of Brandon, Vt., describes the Brandon frozen well queried by the correspondent, "J. K. S."

"I staid over to Thursday noon at Brandon, Vt., to verify in person the strange fact of the frozen well there situated. This wonder, by air-line, is no more than a half mile from the railroad depot, in a northwesterly direction, but by carriage road about one and one-half miles. Being alone I took the shorter route.

To my surprise I found the surroundings at the well in the greatest state of neglect; the house was deserted, and the windlass itself dismantled of its appendages of rope and bucket. The well, by actual measurement, I found to be forty-one feet in depth, and it is surmounted by a neat marble slab. The place is enclosed by a dilapidated summerhouse of unique construction some ten feet square. From a gentleman residing near, I procured a rope and pail, and attaching to the former a weight made the soundings, etc., and finally drew up and tasted the ice-cold water. A lady residing near, said she often gets ice from the well to cool her butter. 'Andrew Trumbly,' said she, 'residing in the village, dug the well, and owned the property at the time, and will give you all the facts in regard to it that any one can.'

The next morning, I called upon the aforesaid Trumbly, who stated that in October, 1860, he dug the well, having shortly previous built a house upon the property. At the depth of 15 or 16 feet through a gravelly loam he struck frozen gravel, the interstices in the gravel being filled with ice. The frozen mass was 16 feet in depth, and underlying this was a substratum eight inches in depth, of a yellow slippery substance, (probably yellow ochre). Then followed gravel the remaining distance, the whole depth of the well being 41 feet, but water rarely rising above the lower edge of the frozen mass. He doubts if there was ever a day since it was dug that it has not frozen over. In winter it made him a vast amount of trouble, as he had no alternative but to descend into it frequently to break or cut the ice for water, and so great was this difficulty that during some winters, he did not use the water at all. A later attempt had been made some 6 rods from the spot for water, which was relinquished on encountering the frozen mass. Trumbly sold the property on account of the cold incumbrance, and the place is considered of very trifling value by the people of Brandon." * * *

CORRESPONDENCE.

To the Editor:—Yesterday afternoon while my wife and I were out driving awhile after sunset the western sky began to take on the brilliant colors now so common everywhere and so much commented on. About thirty degrees from a parallel with the western horizon lay a stratum of thin clouds which presently became a *deep grass green*, tipped at their lower border with *yellow*. The colors remained bright fully ten minutes, and then slowly faded away with diminishing brightness in the west.

Is not this a rare phenomenon? I don't remember ever to have seen it before. How is it to be accounted for? Of all the theories offered for the red sunsets, that attributing it to moisture in our atmosphere seems most consistent with the phenomena. The green clouds may be accounted for in this way: the solar spectrum having been already divided by the moisture diffused through the atmosphere, the clouds in question happened to fall in the line of the green rays, their lower border in the yellow.

Below this came, in the clear sky, orange, then red, while above were blue and violet, the blue being less noticeable against a blue sky, but the other colors were quite distinct.

Quite often have I seen all the colors of the rain-bow in our sun-set skies. These colors are often thrown on clouds which have shown red, orange, yellow and violet, while green, though rare, is equally explicable with the others. As dark approaches, the red end of the spectrum first disappears, then the other colors in order, leaving violet as the last visible color of the "red sun-set."

Sometimes white bands ascend through the violet, dividing it into segments apparently running vertically.

This has been the usual order here for some time when the evenings have been clear.

Will some one now explain why the red end of the spectrum should be nearest the horizon and the violet end the furthest from it?

Very respectfully,

T. E. Murrell, M. D.

Little Rock, Ark., July 8, 1884.

To the Editor:—I read with much interest the communication from Mr. H. A. Hazen, in the July number of your Journal, regarding "air pressure and thunder storms," particularly the paragraph in which he refers to the apparently anomalous phenomenon of a thunder-storm being "ushered in repeatedly by a strong wind from the direction from which it is coming, and this, too, though the wind may have been from an entirely different direction just previously." He mentions two of the many explanations which he says have been presented to account for this phenomenon, and accepts the view of "a greater pressure immediately at the centre of disturbance," as the most satisfactory explanation. But he overlooked one circumstance in connection with thunder-storms, which, it has accurred to me, may be offered as a possible cause and explanation for the seeming anomaly in question. No reference to it has ever been published (to my knowledge) as a cause for the above phenomenon, and may not have occurred to Mr. Hazen, or any of the correspondents who have been agitating the subject of atmospheric pressure in your valuable Journal. I refer to the circumstance of wind on the extreme borders of a storm, produced by a maximum of pressure, as explained in Loomis' "Treatise on Meteorology," page 144, article 275, in which he says: "Near the line of maximum pressure which surrounds a violent storm there is generally but little wind, and on each side of that line the winds are irregular in their direction, but generally tend

outward from the line of greatest pressure. Hence it happens that near the extreme borders of a storm the winds are found blowing in nearly opposite directions, on one side inward toward the storm, and on the other side *outward from the storm.*" The established law of the wind blowing *toward* a storm approaching *from a distance*, still holds true in all cases, and is doubtless immutable, as are all the laws of Nature; but, when in close proximity to an approaching disturbance, may not the outward wind, mentioned in the above paragraph, be a sufficien- cause to account for the apparent anomaly under consideration, and without doing away with, or overthrowing the generally accepted theory of a lower pressure at the centre of a thunder-storm?

I call attention to the above-mentioned fact of outward winds from the borders of a storm, merely as a possible explanation for the wind blowing "contrary to the usual direction," during an approaching storm, as stated by Mr. Hazen; but as the subject is one of much interest and importance to meteorologists, and the "apparent anomaly" doubtless new to many students, it is certainly worthy of careful study and investigation. I trust the agitation of this question will be continued, as it presents a peculiar problem for the meteorologists to solve; and a further discussion will doubtless arouse an interest in the subject which may result in much profitable information, and stimulate original research into the mysterious phenomena connected with the progress and development of thunderstorms.

Yours, most respectfully,

ARTHUR K. BARTLETT,

BATTLE CREEK, MICH., July 25, '84.

The following is from a private letter, but the criticism of Mr. Abercromby's "Wind-reduction" and the query concerning recording instruments are of general interest. We publish it with the consent of the writer. EDITOR.

I have been much interested in Abercromby's article on Wind-reduction, but there are some things which I fail to understand, and if I am not trespassing on your attention, I will set down some of the reflections which the reading of the article suggested to me. After that, I have a question or two in regard to instruments for registering weather changes.

Mr. Abercromby's inquiries as to the "mean direction of the wind" lead him distinctly to the conclusion that, except in the case of *regular* winds, *there is no such thing* as mean direction—a conclusion which, besides expressing in various forms of words, he renders unmistakeable by an instance (on p. 331) where the two methods for obtaining this element which he regards as least unsatisfactory produce respectively 42° and 185°. When the best available methods give results so nearly diametrically opposite to one another, it is well indeed to give the preference to the method which is easiest in working, and would be better still, it would seem, not to attempt any reduction at all.

But is the case really as bad as that? If I were to walk three miles, N. 10° E., in an hour, and then, turning a right angle, ride by railroad S. 80° E. for three-quarters of an hour at the rate of thirty miles an hour, the question, what was the mean direction of my journey, would have a meaning; and few, I think, would answer it N. 46° 52' E. as by the "geometrical" method, or N. 48° 30' E. as by the "arithmetical." And why should the answer be different when the moving object is a mass of air?

The author states distinctly the meaning of the resultant as "the balance of the backs and fores of the wind" (p. 228). Omitting vertical currents (not because we would, but because we must). the direction of the resultant shows whither the mass of air has, on the whole, been carried; or, stating it dynamically, it shows the direction in which the horizontal force of the wind (as measured by its momentum) has, on the whole, been exerted. It does, therefore, represent *some* physical fact, whereas the author himself calls (no doubt justly) the results of the

geometrical and arithmetical methods "analytical fictions."

The author appears to expect of a "mean direction of the wind" that it shall exhibit the property of such quantities as the mean duration of a life insurance policy, or the mean length of a measured distance when these are found from large numbers of separate cases, viz: that the mean shall be the value of most frequent occurrence, while the other values, symmetrically disposed about it, fall off on both sides, as in the "probability curve." Such a property would seem hardly to be expected of a mean of wind-directions for a day or a week only; yet I know not how otherwise to interpret the view of the author when I find him criticizing each of the proposed values of mean direction for a day as a direction "from which the wind never blew." In the case which he discusses, the wind is supposed to blow part of the day from the S. and the remainder from the W., and it seems pretty clear that, unless we are willing to exclude one or the other of those two winds from the consideration, we *must* obtain as the mean a direction different from either. That he expects of the mean of a short period characteristics which depend on the combination of a multitude of cases, would appear again from the instance at the top of p. 233, when, through inadvertence in selecting his illustration, he actually advances as an objection to all methods of obtaining mean direction a statement which is only true of the "arithmetical" method he is defending. *That* would give a mean direction "a long way from either point," but the resultant or geometrical methods would give either S.W. or N. E.—the resultant, according to the direction from which the wind blew *most*, and the geometrical, according to the direction from which it blew most persistently.

And this suggests an objection to the "arithmetical" plan, which seems to me very important, to wit: the kind of result it will give in the case (which, I suppose, is the actual case in the greater part of the regions of variable winds) of a considerable number of winds from all quarters in the course of a month or a year, with however a real, though perhaps a slight, preponderance about some special direction; which preponderance it is the object of the reduction to bring to light. The arithmetical method in such a case would disguise the direction in question, burying it beneath the totally unmeaning prominence which it would give to the point half-way around from the zero point. Add any long column of figures, not specially selected, and the average value of a figure will be found to be not far from 4½ in every case, and so, in averaging the numbers expressing in degrees the bearing of the wind reckoned from N., there would be such a pressure around 180° as would give the impression (should this plan be widely adopted) of south winds prevailing over the entire earth, except in the region of trades and similar winds.

A special case of this objection to the arithmetical method is that it does *not* "fail," (as any method of obtaining a mean direction obviously ought to do) when the winds are equally balanced in opposite directions. If the wind blows during a day half from the N. E. and half from the S. W. the "failure" of the resultant method shows an important fact, to wit: that the backs and fores of the wind completely balance each other, while the arithmetical method, instead of failing, points out S. E. as the mean direction—one which has no better claim than N. W.

The author, finally, shows that if a similar, or nearly similar, law of daily variation in direction holds for all winds, then the arithmetical method of getting mean directions is as good as any for bringing out this law. The above statement (expressed in my own words for brevity) is true if by "similar" we understand that the law of daily change in direction is such as to deflect *toward the same hand* all winds, irrespective of direc-

tion; *e. g.*, that if one day the mean direction of the wind is east, but in virtue of the law of daily change it is deflected at noon toward the north, then, on a day when the mean direction is west, there will be a tendency at noon to deflect toward the south. I would be especially obliged for information as to whether this is the way a law of daily change is supposed usually to operate. I cannot imagine what physical cause would find expression in such a law. If, on the other hand, there is a tendency at noon, or say at 6 A. M., toward a particular *direction* of wind (as in the case of a land and sea breeze), so that an east wind becomes at that hour N. E. and a west wind N. W., then the combination of the east and west wind by the arithmetical method would *destroy* the evidence of daily change.

I do not forget in writing as above that Mr. Abercromby is doubtless intimately acquainted with practical and theoretical meteorology, while I am not. The purpose of my writing is to have my misapprehensions corrected. But as from the nature of things the person who has made a mistake does not know where it is, it is the shortest way out for him to state his view as if it were equally sound throughout, and then his interlocutor can put his finger on the weak point.

I appreciate, too, the force of the argument that results ought to be stated in such a form that it should be possible to separate velocity from direction, so that (for instance) an effect, which depends on either singly, may be traced to its cause.

And I am especially gratified to learn (as a corollary) that where observations of velocity are wanting, something may still be done with a register of pure direction. That is exactly what has been kept here during last winter, for there were only daily observations of the anemometer, while the anemoscope kept up a constant pencil register. I mean to work up that register now, *in re* the editorial opinion that my diurnal change in wind-direction is illusory (AM. METEOROLOGICAL JOURNAL, p. 74), and shall be glad to believe (if I can) with Mr. Abercromby that for that specific purpose the records are full as good, and perhaps better, than if I had been at the great trouble of mixing in the velocity by means of a traverse table.

As to the method to be employed in the reduction I submit the following as the best plan I can think of at present, and would be glad of amendment or suggestion. I will divide the angular space of 360° around from N. to N. again into eight, twelve, or sixteen parts (I think twelve would be a good number), and determine, for every hour of the time examined, in which one of these divisions the direction of the wind, as a whole, fell. If a marked change takes place in the course of an hour, perhaps I had better record the wind which blew during the greater part of the hour, and disregard the other. Would that be better than to record an intermediate point "from which the wind never blew?" Having determined upon the direction for every hour of a particular month, I will make a table, the two arguments of which shall be the hour of the day and the direction of the wind, and so tabulate the frequency of each particular wind at each particular hour, for the month in question. Proceeding in this way, I shall have some six or eight large tables—rather too much for the space of the JOURNAL if the evidence for or against a diurnal variation should be never so clear. They might, of course, be combined by simple addition into one table in which no account should be taken of the time of year, but I have no doubt that the diurnal change, if it exist, varies from month to month, on account of the change in the time of sunrise and sunset, if for no other reason. Hence I should wish to keep the months separate in my final tabulation, and perhaps a good way would be, regarding the tabulated winds of (say) 10 A. M. in January as represented by lines properly directed, and in length proportional to the tabulated frequency, to determine the direction of the *center of gravity* of all these lines from the origin.

(This would be substantially the author's "geometrical" method.) Calling this the mean direction for the hour, I should get a table of mean directions, the arguments of which would be the months of the year and the hours of the day. Would that be as good a form as any in which to present evidence on the question in debate?

I come at length to the question regarding registering instruments. Last winter some citizens of this town raised about $300 for the purchase of such instruments for the college, to be under my care. I had already, and have now, in use the two instruments for the wind, the Signal Service anemometer register (the one which works like a chronograph, recording by means of a voltaic battery each mile of wind as it is completed) and a mechanical device paterned after Professor Draper's, but made by a Colorado Springs mechanic, for the direction; (a vertical cylinder turning with the vane, and a pencil descending along it with a uniform rate of fall.) I want to get a registering barometer, thermometer, and sun-thermometer, and though there are other things I want (as for instance a registering rain gauge), I think the three named are as much as can be paid for at present.

I want to ask you: Are there any instruments as good as Draper's within our means? Whom shall I get to make them for me?

Yours respectfully,

F. H. Loud,

Colorado Springs, Col.